Rogers (R. E.)

EXPERIMENTS

UPON THE

BLOOD,

TOGETHER WITH SOME NEW FACTS

IN REGARD TO

ANIMAL AND VEGETABLE STRUCTURES,

Illustrative of many of the most important phenomena of Organic Life,—among them Respiration, animal Heat, Venous Circulation, Secretion, and Nutrition.

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EXPERIMENTS UPON THE BLOOD.

Since the days of the celebrated Harvey, most of the questions connected with the functions of respiration, animal heat, venous circulation, nutrition, and secretion have earnestly engaged the attention of physiologists. At an early period in the history of these inquiries, they were embarrassed by many difficulties, arising principally from the little that had been experimentally determined respecting the true nature of the fluids and solids concerned in these functions. Even up to the present time the solution of most of the fundamental questions in physiology has been greatly impeded by the very general neglect of chemical methods of research. The close and legitimate connexion which prevails between many departments of chemistry and physiology, would seem naturally to suggest that the latter might derive important benefit from admitting into its discussions the accurate and abundant data furnished by the other. Inattention to this most desirable application of the powers of chemical research to the elucidation of the operations of life, has too often deprived the physiologist of auxiliaries in the interrogation of nature of more value than all the other means within the reach of the medical philosopher. Modern medical literature is crowded with ingenious speculations respecting the various changes going on throughout the system; but, how rarely has the experimental chemist entered the field of inquiry, with the view to lead us by the path of cautious analysis and fair in-

duction to a knowledge of what these changes are. Speculation and conjecture are now giving way, however, to the growing desire for the developement of facts, while the daily advances making in the modes of experimenting and an increase of skill in observation, are doing much to alter the whole aspect of the science. Nothing has so much delayed this important reform in the method of investigating medical problems as the distinction continually drawn between the phenomena of living organized beings and ordinary matter. A prevailing, though I believe mistaken, persuasion, that the laws which regulate the several motions and changes in organized structures have nothing in common with those of ordinary chemical action, but are exclusively obedient to a class of forces distinct in their nature from any which preside over the inorganic world, has invested the whole subject with an air of mystery and obscurity calculated much to retard the progress of medicine. But, instead of placing the actions of the living system under the dominion of certain occult powers, and believing, in consequence, that the seat and causes of these actions are altogether out of the reach of examination by experimental means, ought we not rather to consider them, so far as they are within the reach of human investigation, as differing from the phenomena of inorganic matter only in the peculiar complexity of the structures in which they originate, and the multiplicity of the agencies to which this complexity must give rise. It is true that the chemist, in his researches into life, is not permitted to reach the sanctuary of the presiding genius, yet this is no reason why he should not be allowed to investigate the structure of her habitation. This, we believe, is the legitimate object of chemistry, as applied to physiology.

The rapid progress which analytical chemistry has recently made in the dominion of organic nature, and the light which its discoveries have cast on the phenomena of disease, have made the contributions of Priestley, Berthollet, Berzelius, Davy, Edwards, Brande, and many others, of inestimable value to modern medicine.

The importance of the several inquiries above alluded to, is itself apology enough for any new attempt at exploring this extensive field.

Of Respiration.—Physiologists, long aware of the large amount of blood circulating through the lungs, and familiar with the minute anatomical character of these organs, were led at an early period to attribute to them important functions. New and increased attention was drawn to the respiratory system by the discovery of Harvey, that during each entire circulation the whole mass of the blood passes through the lungs, that it there undergoes the curious change from the venous to the arterial condition, and that for this result to take place

the presence of atmospheric air is indispensably necessary. But the real source of this change and the mode in which it is effected were questions at that time undetermined, and gave rise to numerous conflicting doctrines, founded rather upon hypotheses than derived from ascertained facts. It was not until Black had made known the existence of carbonic acid in the expired air, and the fact that this gas consists of oxygen and carbon, that any thing like a definite or satisfactory interpretation of the phenomena of respiration presented itself. The scientific contributions of this distinguished philosopher gave a new impulse to the subject, and have furnished the foundation upon which the chemical theories, now maintained, have been erected. The following are the two leading theories of the function of respiration, which prevail at the present day.

The first, originally stated by Black, and adopted by Priestley, Lavoisier and Crawford, supposes that venous blood contains a large excess of free carbon, and that, in its passage through the lungs, the carbon coming in contact with the oxygen of the inhaled atmosphere, unites with it and forms carbonic acid, which, by this theory, is considered to be exhaled as soon as generated.

The second theory, that of Lagrange, Edwards and Hassenfratz, was adopted chiefly to explain an apparent difficulty in the above hypothesis of Black, namely, that the excessive amount of heat locally generated in the lungs by the rapid chemical union of the carbon and oxygen, would be sufficient to disorganize their structure. It supposes that the oxygen does not combine with the carbon in the lungs, but that it enters the arterial circulation, there *gradually* forms the union, and is returned along the veins to the lungs in the state of free carbonic acid.

A third hypothesis, advanced by Mr. Ellis, though not generally referred to, differs fundamentally from both the preceding. According to his view, no free carbon whatever exists in the blood; but he supposes it to be evolved from this fluid by a species of secretory process, by which it is made to enter the air cells in a free state, to combine there with oxygen, and to be expired as carbonic acid. I shall again advert to this theory, when I present my own view.

By the theory of Black, carbon is presumed to exist in the free state in venous blood, and to impart to this fluid its dark modena tint. This idea is such as could only have originated during the infancy of chemical science, and is certainly inconsistent with the facts and principles which more recent investigations have disclosed. If carbon exists, as is supposed, in a free state in venous blood, I would ask, under what form is it conceived to be present? To imagine it in the state of a vapour or of a liquid, would be to adopt an entirely

gratuitous conjecture, because chemists are yet unacquainted with it in either of these conditions. Besides, why make blackness an essential attribute of carbon, when the diamond, a purer form than charcoal, ought, if any thing, to have a preference as the type of this substance. The assumed inference, therefore, that the ordinary black colour of carbon has any agency in producing the colour of venous blood, ought hardly to be admitted into philosophical reasonings on this subject. The existence of carbon in the free state in the blood, would, from the known insolubility of the substance, lead us to suppose it capable of separation by ordinary chemical means. It may be true, as some suppose, that, when venous and arterial blood are incinerated, a large proportion of carbon is derived from the former; but this by no means proves that the extra portion of carbon of the venous blood was in an uncombined state. On the contrary, it appears more natural to conceive that, as a constituent of most of the proximate materials of the blood, any difference in the proportion of these in the two kinds of that fluid must occasion the difference in the quantity of carbon in question.

The second view of respiration which has been given, which considers that the oxygen is taken up by the arterial blood, and, while in the circulation meets with the carbon, there forming the carbonic acid, leaves unaccounted for the difference in tint between the venous and arterial fluid. Dr. Stevens, in his treatise on the blood, has endeavoured to furnish an explanation of this phenomenon, and has presented some new views of arterialization. Having found that certain salts possess the power of reddening venous blood, and that alkalies and certain acids darken arterial blood, he supposes that the oxygen, entering the lungs, is conducted by the arteries to their capillary extremities, that it there meets with the carbon, and forms with it carbonic acid, which, returning along the veins with the venous blood, imparts to it the modena tint. Carbonic acid, not carbon, according to this view, is the immediate cause of the dark colour; and the saline matters of the serum, and not oxygen, are conceived to produce the florid arterial hue. Dr. Stevens imagines that the carbonic acid in the venous blood counteracts the reddening influence of the salts there, and he denies that the oxygen possesses any other action than that of removing, by what he terms a "latent attraction," this impurity, the carbonic acid, thus leaving the serum free to exert that influence which he attributes exclusively to saline substances. We perceive, therefore, that the whole question of colour, according to the advocates of this view, rests upon the supposed existence in venous blood of a large excess of free carbonic acid, the presence of which has been frequently maintained, and as often denied.

Blood, immediately after it is drawn, possesses a feeble alkaline reaction, indicated in its action upon litmus reddened by an acid, though turmeric paper seems not sufficiently sensible to show it. After it has reposed out of the circulation twenty-four or thirty-six hours, it is then much more decidedly alkaline, manifesting its action upon the turmeric paper very plainly. These facts are regarded by Dr. Stevens and others as favouring strongly the idea that venous blood contains a large portion of free carbonic acid. That the original alkalinity shown by newly drawn blood proceeds from alkaline carbonates, is, I admit, clearly in consonance with reason and experiment. But that the increase of alkalinity, during repose, arises from an escape of the free carbonic acid permitting the salts to exert their alkaline effects uncounteracted, is a doctrine I cannot accede to. Experiment leads me to the belief that all the alkalinity beyond that originally found when the blood is fresh, is in consequence of the formation of a portion of ammonia. In all instances where I have witnessed an increase of *alkaline* action, I have detected the most unequivocal indications of *ammonia.* The following experiment may serve to establish this conclusion. Two bottles were filled with venous blood caught directly from the vein, and one was forthwith tightly corked, the other left unstopped to admit the atmosphere. After standing six hours, a period supposed to be sufficiently long to allow any free carbonic acid, supposing it to have been present, to escape from the open specimen, they were carefully tested, and not the slightest difference in their degree of alkalinity was perceptible. Tested, thirty hours after, both portions showed a considerable addition to their alkalinity, exceeding the amount indicated by two other similarly treated portions of blood examined after eighteen hours repose.

A glass rod, moistened with muriatic acid, being held over some of the blood first employed, white fumes were copiously formed, indicating the existence of ammonia. In order to be fully persuaded of the presence of this substance, some of the serum was placed in the globular vessel of the apparatus represented in *Fig.* 1, and a very gentle heat applied, with a view to expel any volatile matter. The gaseous matter evolved, was made to traverse a solution *(a)* of litmus delicately reddened by acetic acid. The bubbles, in their passage through the liquid, soon developed its blue tinge. Recently drawn blood was tried in a similar manner, but no analogous effect resulted.

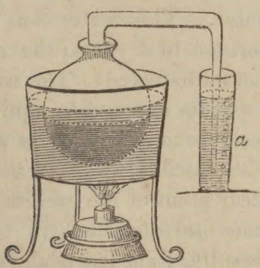


Fig. 1.

Conclusive evidence is thus furnished of the presence of ammonia, and of its elaboration from the blood by decomposition. It is to this process, then, and not to the extrication of any uncombined carbonic acid, the existence of which I think cannot be demonstrated, that we are to attribute the extra amount of alkalinity in blood, after standing. Dr. Stevens, in maintaining a favourite hypothesis, has overlooked, I apprehend, this all-important chemical fact.

Another opinion advanced by Dr. Stevens, is, that oxygen possesses no specific reddening agency farther than that which arises from its fancied power of abstracting carbonic acid from the blood. The following is the purport of the evidence upon which he founds his opinions. He considers that if a portion of a fresh red clot be immersed in distilled water, the saline matters will be rapidly removed by the water, and the clot will become perfectly black; also, that if it be then placed in pure oxygen, it will not return in the least to its former hue. To these statements, I have, at present, one important objection to make: it is, that, by immersing a clot in water, we do not, by any means, remove all the saline matter from the colouring portions and fibrine. This will be seen by adverting to the following experiments. A clot of blood was subjected to a constant stream of water from a hydrant for two hours; after which the washings were examined for saline matter, when they gave decided indications of its presence. The testing was repeated after the clot had been exposed to the current for four, six, eight, and twelve hours, and always with the same results.

The difficulty of separating entirely the saline from the other portions of the blood, induced me to ascertain how far the washing might be pursued without wholly removing all traces of the former. Accordingly, the following experiment was made. A clot was exposed to the stream from a hydrant for thirty-six hours, when the fibrine was detached completely from the colouring matter, and a small amount of the latter was dispersed through the water. This water was decanted to the amount of a pint, and evaporated to dryness; the residue was again washed, leaving the albumen undissolved. The water from this was again evaporated, and its residuum exposed to ignition in a platinum crucible, by which all the volatile animal matters were burnt off. The mass which remained in the crucible was then tested for salts, and gave with nitrate of silver clear proof of the presence of muriatic acid, showing the existence of some *muriate.*

From this and from numerous similar results, I am led to the conclusion, that the presence of colouring matter in the blood is always accompanied by that of the saline substances, while, from some expe-

riments which I have undertaken upon the subject, and the many fruitless attempts I have made to insulate the colouring principle, I am almost induced to believe that there is not in the blood any independent proximate principle, whose *exclusive* province is to give colour, but that the presence of some salt is essential to this result. Must we not, therefore, regard the methods laid down by most authors for the separation and insulation of colouring matter, as erroneous? That of Berzelius, which consists in repeatedly soaking slices of the clot in water, and drying it between bibulous paper, is evidently insufficient to procure the colouring matter alone, while that of Engelhart destroys its character entirely as colouring matter.

The statements of Macaire and Marcet that the *colour* of the *colouring* matter of arterial and venous blood differs, and that the former is not so dark as the latter, as it proceeds upon the assumption that they succeeded in insulating the colouring matter, is obviously liable to objection. I cannot help believing that the difference which they have noticed, is caused, in part, if not entirely, by a difference in the relative amount of saline matter present in arterial or venous blood, the effect of which has not been sufficiently recognised. In some experiments upon venous and arterial blood, made with the view of determining the relative amount of saline matter present in the coloured washings obtained from the clots of each, I found that the icinerated residuum procured from the arterial clot was always richer in this ingredient than the other.

Experiments were undertaken by Sir Humphrey Davy, Vogel, Sir E. Home, Brande, and Scudamore upon blood which was introduced into a vacuum before it had coagulated. It evolved a gas which they conceived to be carbonic acid, but which Dr. Clanny supposes to be nitrogen. In some similar experiments, however, made by Dr. J. Davy, Dr. Williams, Dr. Duncan, Dr. Christison, and Tiedemann and Gmelin, the same results were not attained. We are not informed which kind of blood was employed by these experimentalists. Some of those last mentioned, however, still conceive that carbonic acid exists in venous blood, though it is not extricated during coagulation. My own experiments, conducted with all possible precaution, have always failed in detecting this gas in venous blood, either during coagulation or subsequently; and I find that in this fact I am supported by the testimony of Berzelius, Muller, Mitscherlich, Gmelin and Tiedemann, who maintain that venous blood not only does not extricate carbonic acid while in a vacuum, but that it will absorb this gas to a very considerable amount, not yielding it again even to the air pump.

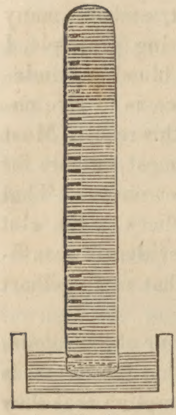


Fig. 2.

For the purpose of discovering if any gas is given off from the blood, I introduced the pipe of a glass syringe into the jugular of a sheep, shutting out the access of the atmosphere by means of a stop-cock, as soon as it was filled. I was thus enabled to pass a portion of blood, without its being exposed to the air, into a receiver over mercury. In this condition, the apparatus (*Fig.* 2) remained for three weeks, during which time not the least depression of the mercury occurred, showing most plainly that no gas whatever had been evolved. This was frequently repeated, and the experiment was extended to blood drawn from the human arm, excluded in a similar manner from the air, and invariably with the same results.

Not wholly satisfied with this negative evidence, I have endeavoured to test the presence of the suspected gas by the action of heat. With this view a little apparatus shown in *Fig.* 3, was employed. A portion of venous blood was allowed to flow immediately from the vein into the bulb *(a)* which communicates by a tube with the vessels *(b)* and *(c)*; *(b)* containing lime water, and *(c)* containing a solution of litmus. A heat of 212° was applied by means of a water bath to the bulb, and the effect of the expelled air or gas upon the liquids was looked for: not the least change upon either of them was produced. The same occurred with blood tried at different periods of repose, and similar experiments made with serum alone, and with crassamentum alone, and also with arterial blood and its portions, always gave the same results, provided those periods were not so long as to have allowed of *animal decomposition*. This experiment was extended to the fluid taken from a patient labouring under ascites, and which as nearly as could be ascertained, closely resembled ordinary serum; and in this instance, as before, the evidence was distinctly adverse to the existence of *uncombined* carbonic acid. In every one of these cases, however, when the water bath was replaced by a sand heat of 260°, carbonic acid was strongly indicated by both the test liquids. But, whenever this was so, there was every proof of decomposition having begun. The generation of ammonia, together with the extrication of muriatic

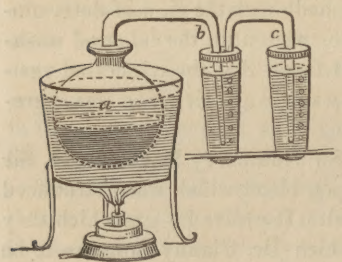


Fig. 3.

acid, ascertained by attaching a third test tube, containing a solution of the nitrate of silver, sufficed to render this certain.

Such, then, thus far, is the unsettled posture of our subject; to all appearance, scarcely more approximated to a satisfactory explanation of the important function here discussed, than in the time of Black.

On the action of Animal and Vegetable Tissues.—Before proceeding with the subject of respiration, I propose, under the present head, to state some fundamental facts which belong to a path of research but recently entered, and hitherto little trodden, furnishing a solution to many of the most important mysteries of physiology, and shedding light where previously all was obscurity and conjecture.

The first steps in investigating the action of tissues were made by H. M. Dutrochet, to whom is due the credit of having originally called the attention of the scientific world to a new and curious class of phenomena, under the appellation of endosmosis of liquids.

Dr. J. K. Mitchell, pursuing with great ingenuity and success a somewhat similar train of research, has much enlarged the limits of the subject, by extending it to gases; and professor Graham, of Glasgow, under the title of the diffusion of gases, has slightly adverted to the latter branch of inquiry, confining his attention more exclusively, however, to the laws which regulate the diffusion of aeriform fluids through capillary orifices.

In reviewing the labours of these experimenters, I have occasionally arrived at conclusions not in accordance with theirs, while, in many instances, I have the satisfaction of finding that our results entirely agree.

The chief and most essential point of difference between the results of the gentlemen mentioned and my own, is in reference to the view which is taken by them, that any liquid or gas invariably follows one direction, in relation to any other particular liquid or gas, without regard to the nature of the interposed structure, which my own experiments convince me does control, not only what particular gas or liquid in each case will be transmitted, but influences likewise the ratio in which they traverse the tissue. These authors would seem to conceive that the interposed substance is passive during the phenomena. It has appeared to me a point of high interest, however, to ascertain whether there does not exist an important influence due to the character of the separating structure.

My first object of attention has been to find whether or not there is a disparity in the rate of passage of different gases through different structures. For this purpose four short tubes (*Fig.* 4,) were chosen, equal in length and diameter. A portion of fresh cuticle recently separated from

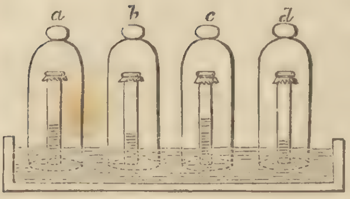


Fig. 4.

the *cutis vera* was tied across the end of the first *(a.)* Over the second *(b)* was fastened a portion of peritoneum; over *(c)* was a piece of mucous membrane; and over *(d)* a very thin section of fresh liver. These tubes being thus prepared and arranged over a mercurial trough, an equal measure of carbonic acid was passed up into each; a glass vessel was inverted over each of the tubes and filled with oxygen six times in volume of the carbonic acid in each tube. The two opposite sides of the organic structures were thus in contact with different gaseous atmospheres. A rise of the mercury in each of the tubes was soon perceived, and the rate of movement was seen to be distinctly different in each. At the end of thirty minutes the experiment was suspended, being deemed satisfactory; and the mercury in the several tubes stood nearly as represented in the figure. We here perceive that from the tube *(c)* where the mucous membrane was used, was the largest escape of the contained carbonic acid: a less proportion passed through the cuticle, *(a)* a less share still through the peritoneum *(b)*, and least of all through the section of liver *(d)*. I should state that, while the mercury was rising in the tubes, they were depressed to maintain the same level both inside and out in order to avoid any interference in the rate of passage from pressure. The tissues employed were as recent as could be procured. These inquiries were repeated and extended to other membranes with similar results, which I do not think it necessary to describe, as the cases given above are sufficient to establish the general proposition. As a deduction from the preceding determination, it seemed highly probable that by the use of certain tissues we might effect a separation of particular gas from a mixture of two or more, so that by varying the tissue we might eliminate any given gas at will, performing a species of approximate analysis. To test the truth of such an inference, two tubes (*Fig.* 5) were taken, and being bent into a rectangular elbow, one extremity of each was closed by a plate of metal perforated by a small round hole, corresponding to the calibre of the tube. A membrane being placed between the plates, they were then tightly clamped together. Thus arranged, I introduced into the leg of one of the tubes, a measure of nitrous oxide, a measure of carbonic acid, a measure of oxygen, and a measure of hydrogen;

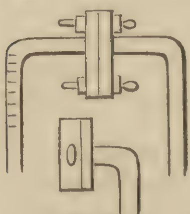


Fig. 5.

and four measures of nitrogen were made to enter the other. The tissue employed in the first instance, was mucous membrane. In fifteen minutes, the mercury stood elevated in the first tube and depressed in the second; and the experiment being stopped, the contents of the latter were examined. It was found that nearly the whole augmentation of volume in this tube was due to carbonic acid. Cellular tissue was now substituted in place of the mucous membrane; and after a longer time than in the previous case, when a similar change had arisen in the volume of gas in the two tubes, the contents of that tube which previously held the nitrogen were inspected, and were found to consist of some carbonic acid, a still greater proportion of oxygen, and all the nitrogen previously present. These experiments were extended to vegetable tissues, the gases being frequently varied, and always the general results were analogous to those above in showing a diversity of action, according to the particular tissue and gases employed.

Another important branch of the inquiry, relates to the existence of a similar property in these organic structures to act on liquids. The extension of these laws of action to liquids is of the utmost importance to physiology; for nearly all the principal functions of animal and vegetable life seem intimately connected with the several specific relationships subsisting between their fluids and solids.

A bottle, filled with carbonic acid, was closed by a piece of mucous membrane, and the mouth inverted in lime-water. The marginal figure (*Fig.* 6) represents an appearance which occurred in this experiment, caused by the precipitation of the lime, as carbonate, from the under surface of the membrane in a thousand minute and beautiful striæ and curls, shooting downwards, sometimes with considerable velocity, and spreading through the liquid. In this instance the membrane was slightly indented; and after ten hours the liquid occupied one-fourth of the interior of the bottle. The inner surface of the membrane was discovered to be coated with carbonate of lime, the liquid within being pure water.

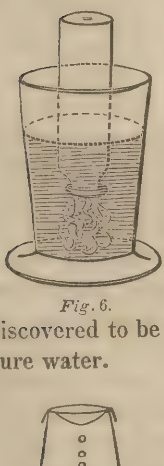


Fig. 6.

A portion of the same membrane was now tied over a glass open at both ends, (*Fig.* 7;) and into the hollow formed by partially exhausting the vessel, some lime water was poured, and found to pass through the membrane unchanged.

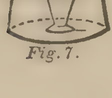


Fig. 7.

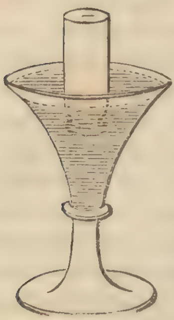


Fig. 8.

A bottle was filled with lime water, and a piece of peritoneum was tied over the mouth, which was then inverted into a weak solution of oxalic acid. (See *Fig.* 8.) In a few minutes white clouds were seen to form and rise within the bottle, showing that the oxalic acid was passing through into the lime water, and that the current established was only in one direction. We thus have evidence of the existence of an agency controlling the transmission of certain fluids in preference to others.

To show that the agency in question is not attributable to ordinary capillary action, and for the sake of corroborating the previous experiment, an arrangement was made consisting of six tubes (*Fig.* 9) of equal diameter, adjusted as in the accompanying figures. The tube *(a)* was drawn at one extremity to a minute capillary orifice; *(b)* was closed with a plug of dense wood; *(c)* was closed by a stucco plug $\frac{1}{8}$ of an inch in thickness; and over the lower ends of *(d,)* *(e)* and *(f,)* portions of peritoneum. Equal portions of a solution of chloride of sodium were put into *(a)* and *(d;)* the same quantities of a solution of sulphate of potash were placed in *(b)* and *(e;)* and in *(c)* and *(f)* the same portions of sulphate of copper.

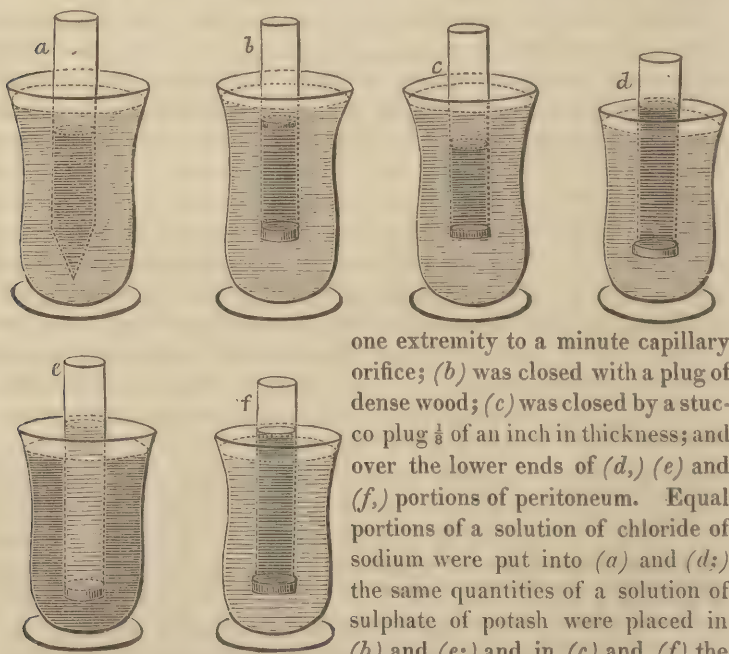
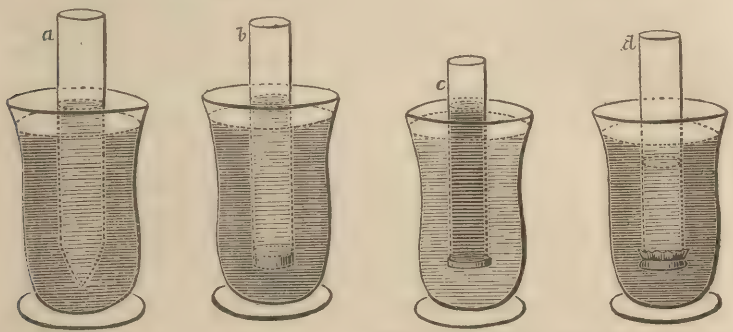


Fig. 9.

The glasses into which these six tubes were immersed, were filled up to the same height with pure water, so that the surface of the

liquids inside and outside of each tube, coincided. After the lapse of six hours, the liquids in the three tubes *(a)*, *(b)* and *(c)*, with the several kinds of capillary termination, had all sunk through a space varying between a fourth and a third of an inch. At the same time, the liquids in the other three tubes stood raised above their former level by about an equal amount.



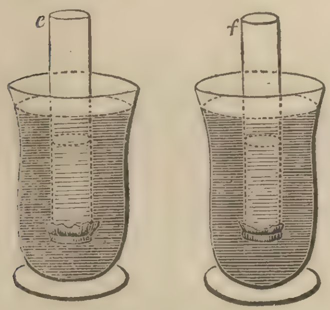


Fig. 10.

The disposition of the liquids was now reversed, water being put into the tubes, while fresh portions of the same solutions before employed were placed outside of them, new pieces of membrane being substituted. As the accompanying figure (*Fig.* 10) shows, the results in this case were the converse of the preceding, the change of level being about equal to the former. The time consumed in this experiment was two hours more than was occupied by the previous one.

By keeping the same liquids and varying the tissue, or by retaining the tissue when the solutions were changed, I have been able to modify these results almost at pleasure. The same tissue in different stages of disease will exert different agencies, as was manifested in the case of an inflamed peritoneum taken from an ascitic patient.

The usual or normal action of a tissue in these experiments, is disturbed by soaking the structure in some astringents. Thus, a piece of cuticle was tried after it had been dipped in a weak solution of tannin, and at another time in a strong solution of alum; and it was found that substances, which before were transmitted very

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readily, were now much retarded in their passage through the membrane.

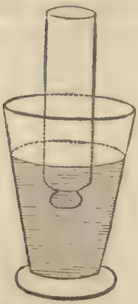

Fig. 11.

Perceiving that, in many instances, the liquids performed their movements in opposition to gravity, I was curious to ascertain if they would be able to overcome a greater mechanical resistance. With this view, a piece of membrane was tied over the mouth of a bottle, (*Fig.* 11,) which was previously filled with a solution of sulphate of potash, and the neck inverted in a vessel of pure water. In six hours the membrane was found greatly distended outwards, the tension increasing for four hours longer, until the cohesion of its parts was no longer competent to balance the peculiar molecular force due to the organic structure, when the membrane burst. By an estimate which is easily made, I find that the force of transmission, in this case, could not have been less than four atmospheres, and we are entitled to conclude that it would have been still greater had the membrane withstood the pressure.

The laws of the transmission of fluids through organic structures, are exhibited in results which are equivalent to a species of chemical decomposition. To render it evident that a liquid will pass through a membrane, when the substance which it holds in solution may be retained, I instituted the following experiment.

One extremity of a tube, (*Fig.* 12,) was fitted with a membrane, as usual: a solution of nitrate of potash was placed in it, and the whole was dipped in a vessel of lime water. After the lapse of some hours, the liquid in the tube was found elevated; and being tested for lime by oxalic acid, it gave no indication of its presence. The water had, therefore, entered the tube, but the lime had been arrested. Comparing this result with that of a former experiment, (p. 14, *Fig.* 8,) we perceive that they differ in one essential point; for, in that first performed, the lime was forced through the membrane by mechanical pressure, in company with the water, while in the other, the membrane, exerting its own influence, unaffected by any controlling power, a separation ensued.

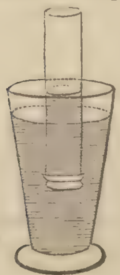

Fig. 12.

By far the most curious experiments which I have performed with membranes are the following:

Prof. J. F. W. Johnson of England, in his report on chemistry,

made to the British Association, states that M. Wache, a German chemist, had succeeded in separating copper and one or two other metals from their solutions by the use of a diaphragm of bladder; but the rationale of the action in this experiment is not mentioned.

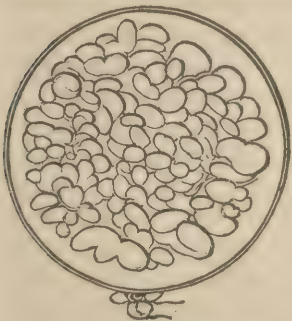

Fig. 13.

My own arrangements for extending the results of Wache, were suggested by this notice. One plan which I adopted, was the following:—a portion of pure water and a few fragments of iron were placed within a tube, over the end of which was a membrane, which was immersed in a solution of sulphate of copper. A deposite of pure metallic copper soon showed itself on the under surface of the membrane, covering it in two days with a beautifully brilliant botryoidal mass, thicker than a cent. The figure here given, will furnish some idea of the appearance of this deposite. By this plan I have separated gold, silver, and several other metals from their solutions. Employing the membrane in the form of a bag to contain the water and the particular metal or other body essential to the action, I have procured, by a simple method, the arbor Saturni and the arbor Dianæ under highly beautiful forms, as seen in *Fig.* 14.

Fig. 14.

In contemplating these experiments, we are led irresistibly to attribute an important office to the membrane itself. It should be mentioned, that not the least trace of the metallic precipitation shows itself upon the inner surface of the membrane, as long as this remains sound and uncorroded; and when, in course of time, some does collect there, it seems not to assume the regular and elegant form so peculiar in the deposite on the outside.

From what we see, then, to attend the normal action of a membrane, the precipitation of the metal externally, and the formation of a salt in solution on the inside, containing the same acid before in combination with that metal, we can hardly resist the conviction that the acid

is liberated on the outside, passing through in its insulated state. To suppose that the sulphate of copper is transmitted undecomposed, seems to require that the copper should be precipitated on the inner side, which is not the case; and to conceive, that, after being abandoned by the acid, it can retrace its way through the membrane, is to imagine a power in the structure more wonderful and incomprehensible than any thing yet presented; for it implies a transmission of a body in a state in which it is considered to be undissolved. An equal difficulty attends the notion that the iron in any condition travels to the acids to precipitate the copper and return as sulphate of iron. Were the membrane clogged with the metallic deposite throughout its substance, the idea of a passage of the metallic matter might receive some support; but there is no interstitial deposite; the mass of copper, on the contrary, is readily detached, in one single piece, from the surface of the membrane, without either rupture or injury, so that it can be again employed in fresh precipitations.

Very different effects from those here detailed present themselves, when bodies of the structure denominated porous are employed. Thus, substituting a stucco-plug in place of the membrane, we find the whole of the deposite to occur on the *inside*, and none at all on the exterior surface, indicating ordinary capillary absorption.

During these experiments with membranes, I have noticed that gas is given off on both sides of the interposed structure; whereas, in the ordinary chemical reaction of the same materials, this evolution of gas is not observed. The nature of the gaseous product I have not yet had sufficient time to determine.

Fig. 15.

To bring this investigation more in contact with physiology, I performed the following experiment. A piece of skin taken from a limb that had been amputated a moment before, was secured across the end of a tube, (*Fig.* 15,) in the manner adopted with the membranes generally, placing the cuticular surface outwards: it was carefully washed and immersed in some pure water. Some freshly drawn blood that had been kept from coagulating by agitation, was put into the tube; and after the whole had remained some hours, the water was examined, and yielded, with nitrate of silver, a distinct white precipitate, showing that a portion of chloride of sodium had been transmitted through it. Thus we see that the mere structure itself of the skin is sufficient to explain the transudation of the saline substances, which, during life, are evolved upon its surface, and, in dis-

ease, accumulate beneath it. Analogy leads me confidently to hope that secretion generally will be found to explain itself by a similar law of action, residing in all glandular structures. I have reason to believe, from experiment, that urea may be artificially procured from blood, by placing this fluid in contact with a slice of kidney. Having obtained, however, but one insulated result, I offer it with that diffidence which the importance of the conclusions dependant on it render necessary.

Of the evolution of heat by organic tissues.—The chief changes of temperature in the inorganic world have been referred by chemists to friction, chemical affinities, and the numerous changes which matter undergoes in its physical forms, such as changes from the solid, liquid and gaseous condition. I am not aware that it has been distinctly recognised or ever alluded to, that mere absorption or any molecular movement not accompanied by chemical change, has the effect of developing heat. The curious results to which I believe I have arrived upon this point, will be found, I conceive, to possess important bearings upon sundry inquiries both in physics and in physiology.

I shall begin with a simple and ready means of showing the evolution of heat by absorption of a gas, and refer to the accompanying figure, which conveys an idea of the manner in which the indications of temperature are procured. The fluid in the globular vessel *(a)* (*Fig.* 16) is water, the upper portion being occupied by carbonic acid, which, as fast as the water absorbs it, is supplied through the tube *(b.)* The thermometer, which was on the differential principle, made the change of temperature very obvious.

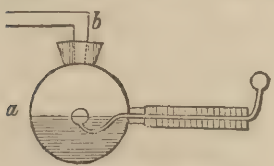

Fig. 16.

For the purpose of experimenting upon the developement of heat produced by the transmission of gases and liquids through structures, I have devised a delicate differential thermometer, in which the fluid is ether. This instrument is represented in *Fig.* 17; *(a)* is a leaden ring, to protect the flattened bulb within it; *(b)* is a paper balloon, surrounding the other bulb, *(t,)* and sheltering it from the interfering influence of the air.

An apparatus was contrived at the same time, enabling me to experiment widely upon the transmission of gases and liquids through organic and other structures. It is constructed with a view to obviate the numerous sources of error from interfering influences, such as

gravity of the fluids, mechanical pressure, difficulties of manipulation attendant upon the ordinary means employed.

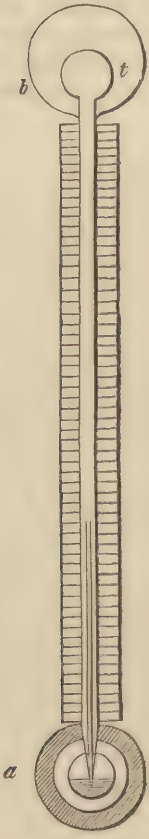

Fig. 17.

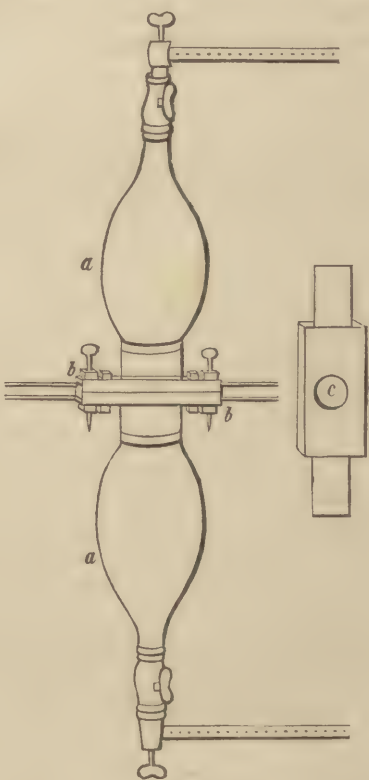

Fig. 18.

This instrument, the same by which I have been enabled to detect an elevation of temperature during the experiments on the movements of gases and liquids through tissues, is represented in *Fig.* 18. It consists of two glass vessels, *(a, a,)* each having its neck fitted with a stop cock, to which is attached a barometer tube accurately graduated. The other end of each glass vessel is closed by a steel cap, containing an air tight slide, both cap and slide furnished with a large round hole *(c)*. The two vessels are firmly fastened together by clamp screws, bringing the two metallic caps in close pressure upon the interposed membrane or structure. In all experiments upon

the evolution of heat, the flat leaden ring of the thermometer described above, (*Fig.* 17,) is placed between these two plates or caps, *(b, b,)* the tissue or structure being also stretched across the ring, and in contact with one side of the flat bulb. By tightening the screws the atmospheric air is excluded. By moving to and fro the slide contained in each cap or plate, a communication between the interior of the two vessels can be established or cut off at any moment. Changes in the volume of the fluids contained in the vessels, are seen by the movement of the mercury in the graduated barometer tubes or guages.

I have not been able, through want of sufficient time, to repeat and extend the interesting results which I have succeeded in procuring by the use of this apparatus, so as to put them out in the form which I consider suitable. I am induced, therefore, to withhold them for the present, in order to have it in my power to offer them at a future day in a digested shape, with the results properly tabulated.

Furnished with a number of fundamental facts derived by the several methods above explained, from direct experiment, we are now prepared to take up the consideration of the several functions which stand at the head of this essay, with a hope, may I be permitted to say, somewhat strengthened, that we are approaching a solution of their proximate causes.

It has been already noticed, in speaking of some experiments performed upon the blood, that neither exhaustion nor heat has the effect of evolving from it any carbonic acid. For the sake of seeing what effect the presence of other gases would have in promoting the separation of the gas here mentioned, five tall tubes, (*Fig.* 19,) sealed at

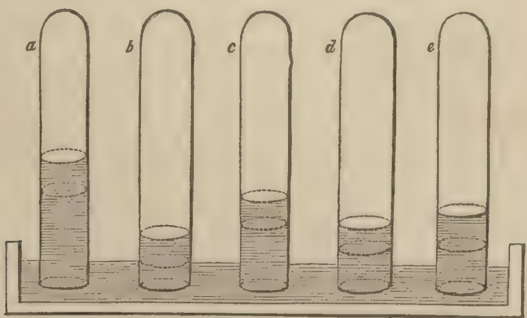

Fig. 19.

one end, were taken, and 200 grains of venous blood passed into each. Into one *(a)* was passed 200 measures of *oxygen;* into *(b)* the same volume of *hydrogen;* into *(c)* an equal bulk of *nitrous oxide;* into *(d)* the same portion of *nitrogen;* and into *(e)* an equal volume of *carbonic acid.* The blood in that tube, in which this fluid was in contact

with oxygen, was much reddened; it was less so in that where the nitrous oxide was; still less where the hydrogen was; while no apparent alteration of colour whatever was perceptible where the nitrogen was present. These conditions continued for nearly eight hours, at which time 40 measures of the oxygen had disappeared, a less proportion of nitrous oxide, a small amount of nitrogen, and no hydrogen that was discernible, but a bulk of carbonic acid, exceeding that of the blood, had disappeared from the tube *(e.)*

Agitating the several residual gases in the tubes with barytic water, not the slightest precipitate showed itself, to indicate the escape of any carbonic acid from the blood. In all those instances, where an absorption of the gas by the blood took place, there was a decided developement of heat. This can be illustrated by referring to an experiment in which oxygen is employed in contact with blood in a vessel (*Fig.* 20) having, as seen in the figure, a cap through which a thermometer can be made to slide. The tube being partially filled with oxygen, and allowed to repose some time over the mercury, in order to acquire a settled temperature, some venous blood which had stood some time over mercury for a similar purpose, was transferred into the tube until it filled nearly all the space not occupied by the oxygen. Before the introduction of the blood, the temperature of both it and the gas was 60°; but an absorption of oxygen very soon ensued, and after 15 minutes the thermometer stood at 70°, when the experiment was discontinued.

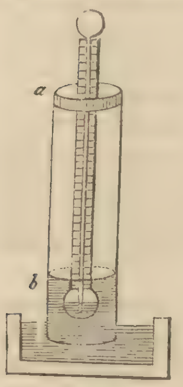

Fig. 20.

A small bladder, (*Fig.* 21,) not long taken from a pig, was filled with fresh venous blood, when it was closed and suspended by a thread from the cover of a tall receiver, which fitted air tight. The receiver standing over mercury, was then filled with oxygen, and in two hours the mercury in the bottom of the receiver was considerably depressed. Upon inspecting the contained air, a very sensible quantity of oxygen had disappeared, but was replaced by a still larger amount of carbonic acid, the excess of which explained the depression in the mercury. This experiment was varied by making trial of other gases, as hydro-

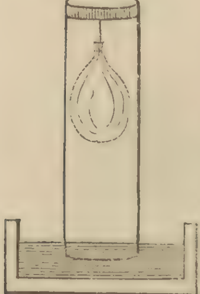

Fig. 21.

gen, nitrogen, and bicarburetted hydrogen; and in every case with a developement of carbonic acid.

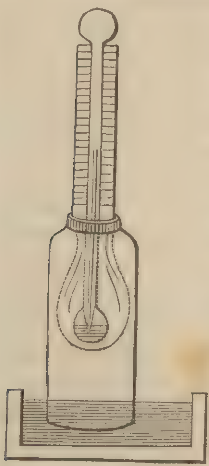

Fig. 22.

In order to ascertain if, during this evolution of carbonic acid and disappearance of the other gases, any elevation of temperature ensues, an apparatus, such as is seen in the margin (*Fig.* 22,) was made use of. Using my form of Sanctorio's thermometer, described above, it was plunged to the bottom in a bag, consisting of membrane, and full of blood, the bag being tightly secured at the neck, and suspended in an atmosphere of oxygen. In this instance the result was in a high degree satisfactory.

A considerably greater rise of temperature was manifested, proceeding no doubt from the joint influence of the absorption of oxygen and the transmission through the membrane of carbonic acid.

Let us now recapitulate the leading facts developed in regard to the evolution of carbonic acid from venous blood.

1. Exhaustion by means of the air pump has no effect in evolving carbonic acid from venous blood.

2. A temperature of 212° displaces no carbonic acid from venous blood.

3. The carbonic acid, on the other hand, is *absorbed* by exposing it to venous blood.

4. Exposing venous blood to oxygen, nitrogen, hydrogen and nitrous oxide, through each of these, if we except hydrogen, is in part absorbed, yet not a particle of carbonic acid is given off.

5. When, however, a portion of venous blood is placed in a bag of some membrane, and the whole immersed in an atmosphere of some gas, oxygen, hydrogen, nitrogen or bicarburetted hydrogen, (others have not yet been tried,) then carbonic acid is pretty freely evolved.

What a beautiful solution of the problem of respiration is presented in the facts here announced. The close accordance between the conditions of the last described experiment and those embraced in the function should be adverted to.

Among the mammalia, the lungs at each inspiration are supplied through the bronchial tubes with a large amount of atmospheric air, while the pulmonary veins, ramifying over their parietes, convey to them a large supply of blood. There, opposed to one surface of the

tissue of the lungs, is an atmosphere of oxygen and nitrogen; and, in contact with the other, venous blood; so that, precisely as in the experiment, carbonic air becomes eliminated at each expiration.

Comparing the large volume of carbonic acid expelled during every contraction of the lungs with the small quantity procured in my experiments, it might at first sight seem rather strange that so large an amount should be produced during respiration, from the cause assigned. But our surprise vanishes as we take into contemplation the disparity in the amount of surface brought into operation in the two cases. The whole area of the external surface of the bag did not exceed eight square inches, while it is computed that the mucous membrane of the lungs presents to the air a surface of not less than 1200 square feet, folded into innumerable cells, and in contact with the oxygen and the blood, between which it appears to be the means of maintaining a most curious species of connexion. Adopting the fundamental views established by the experiments described in this essay, we are furnished with a simple and satisfactory solution of the manner in which fishes and other aquatic animals having gills, are enabled to separate the respirable element from the fluid in which they live.

We behold, in like manner, how the whole difference in the mode of respiration of animals and plants may be the result of some simple contrariety in the intimate structure of the animal and vegetable tissues, allowing, as we see, oxygen to pass inwards and carbonic acid to go outwards, and in plants effecting just the reverse.

Whether the carbonic acid exists in the venous blood in a state of combination in the condition of carbonic acid, or whether only its uncombined elements are present previous to its elimination as a gas, are points beyond our reach to determine.

It does not appear by any means certain that the oxygen taken into the lungs, at any one given respiration, is essential to the formation of the carbonic acid evolved immediately after; for in some of my experiments it was found that both when carbonic acid was disengaged and when it was not, a portion of oxygen disappeared, and in other cases carbonic acid was given off when portions of hydrogen, nitrogen, and bicarburetted hydrogen were absorbed, though none of these contain any oxygen, nor can it be supposed to furnish that element in any manner to that gas. Some researches of Edwards, in which small animals were caused to breathe atmospheres of hydrogen and nitrogen, rendered the notion that oxygen is indispensable still more impossible, because, while they continued to respire at all, they expired carbonic acid; and arresting the experiment before the animals were destroyed, it was found that each had eliminated a volume of this gas

equivalent to its own bulk. I have extended these experiments to bicarburetted hydrogen, and with corresponding results. It would seem, as before intimated, that the extrication of carbonic acid is rather an action belonging to the tissue in virtue of its organized structure, so that the conjecture of Ellis, however erroneous in point of fact, which attributed to the lungs a power of secreting carbon, was an important step, approximating us to that view which recognises in the lungs a function somewhat analogous to that of a gland.

Source of heat in Animals and Plants.—The origin of the heat developed during animal and vegetable life, has been as much a matter of controversy as the subject of respiration itself. Any one who will be at the pains to trace all the innumerable theories and conjectures which have been invented to explain it, will find in what an unsatisfactory condition the whole doctrine rests at present.

Some have considered the chief source of animal heat to arise from the production of the carbonic acid given off during respiration. Others have imagined it to come from various chemical changes presumed to occur in the system. Some explain it by digestion; some by circulation; while some call in the nervous influence or innervation. If we except the first of these explanations, which has called forth much discussion on both sides, we find that the advocates of the several hypotheses enumerated, have done but little to trace animal heat to the causes which they assign. I believe I may be allowed here to say, that the important facts upon this subject, which I hope I have succeeded in developing, have hitherto entirely escaped attention. The manner of establishing the heating influence of transmission through tissues, I have already detailed; and a still more comprehensive law of all organized structures may now be stated—that whenever a structure, in virtue of its molecular forces, causes a passage of a fluid through it, an elevation of temperature ensues. It flows as a legitimate deduction from this law, that heat must be eliminated every where throughout the animal or plant; for, scarce a function can be executed in any part of the system that heat will not be developed. Thus, during respiration, a tissue is traversed by oxygen entering and carbonic acid passing out from the blood. So in its round of circulation, the blood crossing from the remote extremities of the arteries to those of the veins, through an intervening mass of structure, must, in this case, also develope an increased temperature; while, in like manner, during nutrition, a similar result obtains by the introduction of nutrient matter into the circulation, and its final assimilation; for in this, as in the

preceding functions, fluids are incessantly traversing the tissues of the system.

Venous Circulation.—Great difficulties are acknowledged to attend the explanation of the circulation of the blood through the veins. The principal causes assigned have been the contraction of the heart, the suction power of the heart, the contraction of the veins and arteries, and the action of the capillaries.

My own experiments proving the existence in the membranes of the body of a motive power over the liquids of the system, capable of overcoming great mechanical resistance, seem to present us with a new agency more adequate than any hitherto adduced to explain the phenomenon.

Secretion. No direct vascular communication has been detected by anatomists between the arteries and veins of glandular structures. They trace them no farther than to the parietes of the acini; and it is not probable that any continuous connexion between the systems of vessels prevails. All the operations of organic life are executed in the minute structure of the organs. For the production of any change upon the blood, it must be subjected, therefore, to the modifying agency of this minute structure. The office of the vessels seems to be little else than that of a series of conduits distributing the fluid to appropriate parts of the system, where it is brought within the elaborating influence of the structures. We may regard, I think, every tissue as a species of glandular structure, in the proper sense of that term, inasmuch as we find that each elaborates a secretion peculiar to itself, whether it be recognised as a gland or not. For the synovial fluid is as much the peculiar secretion of the synovial membrane, and the mucous fluid as much the peculiar secretion of the mucous membrane, as bile is the appropriate secretion of the liver. The chief difference is, that, in the so called glands, the products are more apparent from being more insulated. The experiments in which substances were separated from their combinations will enable us readily to understand how this function may be performed in the system.

Nutrition.—The experimental researches detailed in the earlier part of this essay, afford us aid in explaining the manner in which nutrition is effected, equally satisfactory with that which it gives of the other functions.

We have seen that tissues possess a power of transmitting certain substances in preference to others,—in fact a separating or selecting

agency; and it is interesting to notice that, very recently, professor Daubeny has made some curious experiments upon the selecting power of plants in regard to earthy matters. We are enabled by these facts, to understand likewise the determination of particular medicines to particular organs, a subject which opens a rich and ample field for future investigation.

As another important application of my experiments, I may allude to the light which it casts on placental circulation. The absence of any discoverable vascular communication between the maternal and fœtal portions of the placenta, has seemed an insuperable difficulty in comprehending its functions; but all necessity for a vascular connexion is dispensed with, if we admit the power of a mere tissue to select and transmit liquids through its substance by an inherent force.

A further developement of the facts and principles which have been presented in this essay seems destined to remove the obscurity in which the explanation of them and other functions has hitherto been involved, and to throw a broad and clear light upon most of the important phenomena of the living organization. The experiments which have been detailed, form only a part of a series of investigations, by which it is hoped, ere long, to establish principles of fundamental importance in physiology, and to illustrate interesting collateral inquiries in physical science. In thus glancing at the important results which have been obtained, minute accuracy in the estimation of quantity or volume has not been attempted: but it is proposed, at a future day, to present an ample and varied detail of experiments, conducted with a view to minute precision, and the developement, if possible, of the numerical laws of the phenomena in each case.

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